

CONCERNING THE PROPOSED JOINT APPOINTMENT OF MARVIN L. MINSKY
BY THE COMPUTER SCIENCE DIVISION AND THE MEDICAL SCHOOL

I have known Marvin Minsky since we were graduate students in mathematics at Princeton. At that time both of us had Artificial Intelligence as a main scientific interest but, whereas I took the easier path of writing a Ph.D. thesis in the conventional field of differential equations, Minsky wrote a thesis on nerve nets. The thesis discusses two topics. The first is the problem of determining what combinations of neural elements and what kinds of connections provide general-purpose computing capacity in the sense of Turing. The results showed that a wide variety will do, in fact that essentially any non-monotonic function can be used to generate a negation or inhibitory property. Therefore the mere fact that a proposed neuron model has general-purpose capability is not much evidence for its biological applicability. The thesis contained also some theories of how reinforced learning behaviour can be obtained from large, partially random nets. This was the first detailed study of this problem.

The thesis contained a description of experimental results on nerve-net learning done in the summer of 1951. While a graduate student, Minsky built the first learning machine, called SNARC, constructed out of artificial neurons. Each of the 40 neurons was a vacuum tube and thyatron circuit with a probability of transmission of a pulse dependent on the setting of a potentiometer. The neurons could be wired into a net which accepted stimuli and gave responses. The experimenter could "reward" or "punish" the machine according to whether he liked the responses and this had the effect of increasing the probabilities of those pathways used in producing "successful" actions and reducing the probabilities of those associated with "unsuccessful" actions. The results showed that the 40-neuron network could be trained, but also that it did nothing particularly unexpected. Minsky did not publish a separate paper on it. Similar models have been published by others recently. Minsky eventually concluded that the construction of large, random neural networks was not to be the path toward good theories of intelligent machines, although eventually such work might become valuable in forming detailed theories of the brain.

My next important contact with Minsky was at the Dartmouth Summer Research Project on Artificial Intelligence in 1956. The most important outcome of this project was Minsky's proposal to reduce tree search in theorem proving programs by having the computer test proposed subgoals on examples. In order to test this idea he proposed a program for proving theorems in plane geometry. In a long report he outlined what could be expected of such a program. His proposals were subsequently carried out by H. Gelernter of IBM. Previous theorem programs, e.g. Newell-Shaw and Simon, spent much of their time in vain efforts to prove false "lemmas" that would lead to the theorem for which a proof was sought.

His next major effort concerned learning in nerve nets and a simple type of learning called hill-climbing. He and Oliver Selfridge wrote several papers in which they clarified substantially what one could and could not make a machine learn by these techniques.

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Perhaps Minsky's most important paper was "Steps Towards Artificial Intelligence" which appeared in 1961. In this paper, he pointed out, with a moderately detailed example, that the early methods of pattern recognition, notably "template matching" and "property lists" were bound to prove to be too weak to help solve the more difficult problems in building systems with powerful perceptual abilities. Instead, one needs a more flexible system for descriptions of things, in which the structure of the description reflects some properties of the thing, rather than being based only on "proper names" of a fixed set of functions or properties. This idea resulted in a number of student theses, written under Minsky's direction, which combine to form an important set of results in the pattern recognition area. A Master's thesis of R.Canaday solved a problem of separating overlapping line drawings, and completing the partially obscured parts of the figures. Another thesis, of W.Teitelman, learns to recognize a large variety of hand-printed figures, this time using also some of Feigenbaum's methods. In a recent Ph.D. thesis, T.Evans develops the description idea quite far, resulting in a program that does surprisingly well on intelligence tests of the "geometric analogy" type. The recursive figure-description idea is at the heart of the recent Ph.D. thesis of Sutherland, in which an engineering "Sketchpad", based on use of the light-pen, is developed to a near-practical point.

In discussing Minsky's work, I must state that both he and I have devoted a good part of our time since 1958 to a project called the "Advice-Taker" which we both believe to be the most promising long-run approach to making a major advance in artificial intelligence. We have found this problem very hard. Minsky claims that he is making progress but does not expect it to show for another couple of years, at least. His main results so far have been discovering that several different attempts led to different difficulties, and he thinks that he has accumulated almost enough conditions to be able to say what has to go into the system. He and I agree that it is necessary to tackle directly this very hard problem, because work on easier problems (e.g. in learning and pattern recognition) over the past few years has led to considerable success there but has shed little light on this.

Minsky has completed large parts of a book on Artificial Intelligence, but he says that it would take him several months to get it in shape for publication and this never seems to be available.

Minsky has very broad interests and competence apart from Artificial Intelligence, and it is very difficult for me to do them justice since I have not followed them in detail. Here are those activities I know about.

1. While a junior fellow at Harvard he developed and patented a microscope with improved resolution and contrast. It worked by vibrating the table containing the specimen in synchronism with the film holder and that it allowed the use of an optical system corrected for axial rays only.
2. He published several papers on the theory of automata. The main result was that a two tape automaton that cannot write can nevertheless be universal. As a consequence he was able to show that the "tag" problem posed by Post in 1922 was unsolvable, and he was also able to obtain a universal Turing machine with only 4 internal states and 7 symbols. This was a major improvement on previous results. Tritter, using Minsky's methods, has since found a 4×6 universal machine. The paper on the "tag" problem was considered

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a very notable result by the logicians. D.Scott has used it to get a direct proof of the Moore, Wang, Kahr result on AEA. Wang has published a number of notes related to it. The recent Sheperdson-Sturgis ACM paper devotes considerable attention to it, and is really based on expanding the same general ideas.

3. He has written a book on automata which is about to be published by the Benjamin Company.
4. He played an important role in the M.I.T. plans for a large memory time-sharing computer.

Besides his research ability, Minsky can offer the Computer Science Division four other important assets.

1. He knows more about what has been done and is being done in Artificial Intelligence than anyone else. He is an editor of the JACM and he is editor of the artificial intelligence section of Computing Reviews.
2. He is an excellent graduate teacher and supervisor of thesis students. At M.I.T. he has about 75 students in his course each spring and that these students get a lot out of his course is shown by the excellent term papers they turn in. I was particularly impressed by the latest batch. He has about six students within a year of a Ph.D. At least three others, J.R.Slagle, P.Abrahams and I.Sutherland, have already earned Ph.D's. His coming to Stanford will bring many students to Stanford and into Computer Science.
3. He is better acquainted with the connections between computer science and biology than anyone else I know of. He understands both theoretical biology and biological instrumentation, and so will be very valuable in helping the medical school learn to use computers. Lederberg has commented on Minsky's remarkable grasp of modern biology.
4. Instrumentation. Minsky has considerable experience in both the electronic and the mechanical aspects of instrumentation, and this will be a considerable asset in helping introduce on line computation into the Medical School instrumentation. He is very strong on practical electronic circuits and is also an experienced machinist. While a Junior Fellow at Harvard he had his own laboratory set-up in Lyman Laboratory. There he did all the precision machining and electronics in the construction of the scanning microscope mentioned above. He also did some interesting mini-machining work on making small hydraulic computer elements using a pantograph engraver.

Other Considerations

A long delay in offering Minsky a position might jeopardize our chance of getting him. Hao Wang is trying to get him to come to the Harvard Computation Laboratory.

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The question of whether Stanford should have two people in Artificial Intelligence or even in non-numerical computation has been raised and should be met squarely. We have the opportunity to make Stanford one of the most important centers of research in computation in the world. To do so we will need not only Minsky, but also someone in the area of computer system design, and additional people in programming and in applications as the opportunity arises. It was my impression that the decision to make Stanford such a center had been made.

John McCarthy

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